ocular organ shines with an ultramarine light, whilst the middle of the five ventral organs is sky-blue and the anal organs are ruby-red. It may also be observed that even in preserved specimens, when examined in a strong light, the different organs seem to shine with different colours, although there is under such conditions no actual emission of light. Furthermore, in some forms (*e.g.*, *Calliteuthis*) there are chromatophores in the superficial layers of the integument over the luminous organs, through which the light admitted must pass. A somewhat similar arrangement obtains in the curious structures in *Chiroteuthis*, which were regarded by Joubin at the time of their discovery as "thermoscopic eyes," but which are, I think rightly, in the present state of our knowledge, considered to be a special kind of luminous organ. In these instances the function of the superficial chromatophores may be to colour the light which passes through them.

The question of the utility of these variously coloured lights to the creature possessing them admits of an answer which is, at all events, extremely plausible. It was suggested in the case of deep-sea fishes by Brauer, and has been adopted by Chun in reference to the Cephalopoda. They serve as recognition marks by which the various species can identify their fellows; just as certain colour patches in the plumage of birds enable them to find their mates, so in the darkness of the ocean abysses do these fairy lamps serve their possessors. Another and perhaps even more obvious utility is suggested by the general distribution of these organs. It has above been pointed out that they are, almost without exception, on the ventral aspect of the body, that is, the inferior surface in the position in which the animal habitually swims. It must happen, therefore, that when the creature is moving over the floor of the ocean in the quest for food, this must be illuminated by its lamps, and the advantage of a series of searchlights playing over the ground will be at once apparent.

Finally we have the question: How is the light produced? To this we can only say that this is an instance of the transformation of one kind of energy into another. We are quite familiar with the production of heat in the animal body by the processes of oxidation which go on in it; we are also familiar with the production of kinetic energy when a muscle contracts under a nervous stimulus; and we are also aware that electric discharges are produced under similar conditions in certain organs of the Torpedo and other fish. The production of light is a phenomenon of the same kind. When we can explain how stimulation applied to a nerve causes contraction in a muscle, then, and not till then (so far as I can see), shall we be within reasonable distance of explaining the action of these living lamps.

One point is worthy of notice which has been ascertained, not by experiments on the Cephalopoda, but on other animals, namely, the remarkable economy of this illuminant. A perfectly infinitesimal proportion of the energy expended is wasted on the production of heat. From this point of view animal phosphorescence puts to shame our most modern devices. Whether we shall ever be able to rival Nature in this respect remains to be seen.

We have thus shown how rapid has been the growth of our knowledge regarding the distribution and structure of these fascinating organs, and yet how little we have learned of the mode of their operation, and we end, as all scientific inquiries end when pursued far enough, with a confession of ignorance.

What I have ventured to lay before you are a few of the fruits of the little garden plot in whose culture I have been privileged to take a humble share. If it has appeared to you that the labour spent upon their production by a few enthusiastic workers has been well expended; if they show that in this, as in any other group of animals, the study of small details conscientiously carried out leads to problems of the deepest interest, my object in the preparation of this Address will have been fully achieved.

MATHEMATICS AND PHYSICS AT THE BRITISH ASSOCIATION.

ON Thursday, August 1, at 10 a.m., Prof. A. E. H. Love, F.R.S., read his presidential address, which has already appeared in full in these columns (see NATURE, August 1). A vote of thanks, moved by Sir D. Gill and seconded by Sir G. Darwin, was carried with acclamation.

The Hon. R. J. Strutt commenced the ordinary proceedings with a paper on helium and radio-activity in common ores and minerals. He was inclined to attribute the helium which can be obtained from minerals, not to a radio-activity of the rocks themselves, but to the radium which they contain. The evidence on which this conclusion was based is that the ratio of radium to helium present is nearly constant. A great exception occurs in the case of beryl, which shows no radio-activity, but contains a large quantity of helium. Prof. Rutherford suggested that thorium should be looked for in beryl as a source of the discrepancy. In his reply, Mr. Strutt stated that he had found thorium in granite but not in beryl in sufficient quantities to afford an explanation of its peculiar behaviour.

Lord Kelvin followed with a paper on the motions of ether produced by collisions of atoms or molecules containing or not containing electrions. To him it seems extremely improbable that differences of grouping atoms all equal and similar should suffice to explain all the different chemical and other properties of the great number of substances now commonly called chemical elements. The impossibility of the transmutation of one element into any other he declared to be almost absolutely certain. The ether he takes as an elastic, compressible, non-gravitational solid. It is, however, only under the *enormous* forces of attraction or repulsion exerted by atoms on ether that augmentation or diminution of its density is practically influential. Purely dynamical reasoning leads him to infer generally similar theorems for an atom to those worked out by Heaviside for an electron. The association of atoms with electrions (or atoms of resinous electricity), and the interaction of both with the ether, form the basis of a general explanation of physical phenomena.

In a paper on secular stability, Prof. Lamb explained the difference between ordinary or temporary stability, *i.e.* stability as asserted by the method of small oscillations, and secular stability, *i.e.* stability when account is held of possible frictional forces; and he gave an experimental illustration of the latter kind. A pendulum hangs by a Hooke's joint from the lower end of a vertical shaft which can be made to rotate by a pulley with constant angular velocity ω. The effect of the rotation is that its two circular component vibrations have different periods, that one being the faster the direction of revolution of which agrees with that of the shaft. The criterion of secular stability imposes a limit to the speed for which the vertical position of the shaft is stable; for speeds higher than the limiting one a new position of equilibrium is possible in which the pendulum rotates at a constant inclination θ given by $\cos\theta = Mgh/(A-C)\omega^2$, where A and C are the two principal moments of inertia of the pendulum at the joint.

The beginning of the session on Friday had been allotted to a discussion on the constitution of the atom, and the committee of the section had not been in error in expecting that this would be of intense interest. Prof. Rutherford, whom we now have permanently in this country, opened it with a speech which was specially intended to suggest lines for discussion rather than to be a dogmatic statement of his own views. It was perfectly clear, however, that he regarded the electron as having come to stay, although at present it is impossible to decide whether the electrons which are set free in radio-activity or are revealed by the optical properties of an atom are merely an outer circle or are a revelation of the internal constitution of the inner core of the atom. He declared in favour of a kinetic view of the atom in opposition to statical views such as that developed by Lord Kelvin. Only on a kinetic theory could the great velocity of the β particles be explained. Sir O. Lodge in his contribu-

tion to the discussion took up a very definite line: "The electrical theory explains both inertia and radiation; and when a satisfactory explanation is given it ought to be supposed to be the right explanation, inasmuch as it is unlikely that two different systems will both fit the facts." One difficulty that has to be faced is that dispersion, absorption, and radiation (which are all connected) appear to indicate that the electronic constituents of an atom are few, but the electrons so tested may be only those which are comparatively free, and are not completely encased or submerged in an opposite charge. Such deep-seated or constitutional electrons would be inaccessible to light vibrations, and would take no part in dispersion unless violently shaken by chemical clash. The real difficulty is our present ignorance of the nature of positive electricity. Sir Wm. Ramsay urged that the chemist is at present more interested in the detachable electrons, and leaves the nature of the constitution of the atom as non-essential from the point of view of chemistry. These detachable electrons are the cause of valency, and chemical elements of the metallic class must be regarded as compounds; for example, HE may represent an atom of hydrogen the E of which is an electron; for hydrogen chloride, dissolved in water, gives as one of its products H, while the E is transferred to the chlorine atom. He referred to Prof. Rutherford's statement that no profound change had been discovered to take place in matter by the *loss* of electrons, and he pointed out that nickel under combined bombardment by electrons is partially transformed into a radio-active body belonging to that series of metals which yield insoluble sulphates. A *gain* of electrons, therefore, produces a permanent change in matter.

To Mr. F. Soddy, the philosophic unification for which Sir O. Lodge had pleaded seemed "unphilosophic and strained." It was possible to retain the idea of the inertia of electricity as being due to the magnetic field around the moving charge without taking the further step of supposing that matter was ultimately electrical in character. The subject is being approached from both the speculative and experimental sides, and the hypotheses of the former side often failed when subjected to the supreme test of prediction, and were sometimes little more than an ingenious mimicry of known facts. Mr. G. A. Schott considered that it was too soon to expect the mathematical physicist to predict new phenomena, since it is only within the last few years that he has been supplied with the materials necessary for his method, *e.g.* the electron. He adduced one positive result which may help to decide between the static and the kinetic view of the atom. A uniform magnetic force acting on a *rotating* ring gives a periodic mechanical force capable of producing resonance. In consequence, a rotating ring is capable of yielding a magnetic moment very much larger than it would do if at rest relatively in the field. Prof. Larmor claimed the right of physicists to make hypotheses even with regard to the atom. He considered that our views about the electron would have been just the same as they now are if radio-activity had not been discovered, and he gave a short historical account of the development of electronic theory from Faraday through Maxwell to modern times. Lord Kelvin preferred to regard the atom as a big gun loaded with an explosive shell. The firing of a shell does not cause the destruction of the gun; the electron, however, changes its nature in a way analogous to the bursting of the shell after explosion.

The discussion throughout was very stimulating, even if few decisive statements could be made. It was noteworthy that no allusion was made to the latest optical papers of Drude, in which he allocated the parts played by bodies the mass of which is that of an electron, and bodies the mass of which is comparable with the mass of an atom, in determining the optical dispersion of solids, *e.g.* fluorspar.

A paper was next read by Sir Wm. Ramsay detailing the remarkable discoveries announced in a letter to NATURE, July 18. Briefly stated, it appears that helium, neon, or argon is formed from radium emanation according as it is dry, dissolved in water, or dissolved in a strong solution of a copper salt. Simultaneously, lithium and perhaps sodium are formed, the presumption being that they are formed at the expense of the copper. The importance of the former discovery is that this is the first time the nature of the products of radio-active disintegration has been found to be controllable.

In a paper by Mr. F. Soddy and T. D. Mackenzie (Carnegie research scholar) on pseudo-high vacua, it was shown that the electrical characteristics of a high vacuum occur in helium (purified by calcium and subjected to further purification by the passage of the discharge) at pressures between $\frac{1}{3}$ mm. and $\frac{2}{3}$ mm. of mercury, whereas in hydrogen the same holds at $1/25$ mm. These pressures are far higher than is commonly supposed. The absorption of helium, argon, and neon in spectrum tubes after continuous running occurs mainly in the volatilised film of aluminium deposited from the electrodes. The gas can be mainly recovered by dissolving the film in mercury or heating the tube.

Prof. Larmor gave a very brief summary of a paper on the range of freedom of electrons in metals. It was remarked that a hopeful plan for elucidating the mechanism of the transfer of electricity (electrons) from molecule to molecule is to study the time relations. The optical phenomena of metals introduce times, viz. the periodic times of the vibrations which are small enough for this purpose. Hagen and Rubens's experiments on the connection between infra-red radiation and electric conductivity show that the time required to establish conduction completely is a small fraction of the period of such waves. If the semi-free electrons to which conduction is due have a velocity of mean square determined by the gas laws, this restricts their range of freedom almost to the interspaces between the molecules. On the other hand, the fact that the square of the quasi-index of refraction of light for the nobler metals is not far removed from being a real negative quantity, indicates that the number of such free electrons is of about the same order of magnitude as the number of molecules.

The proceedings on Monday, August 5, opened with a paper by Dr. L. Holborn on optical pyrometry, in which he outlined the various radiation methods of measuring temperature. The most recent optical experiments give for the melting point of platinum the value 1790° C. if the melting point of gold (1064° C.) is taken as the fundamental point. Prof. C. Féry followed with a discussion of the various difficulties which are met with in connection with the subject. He mentioned that an apparatus had recently been devised in which there is nothing electrical. In this the thermometric receiver, instead of being a thermo-element, is a bimetallic spiral which deflects a pointer over a scale attached to the instrument.

Dr. Harker, who had in recent years obtained a considerably lower value for the melting point of platinum, pointed out some of the defects to which the optical method was liable. In particular, there is an uncertainty arising from the absorption of the light by the vapour given off the walls of the furnace.

After the end of this discussion the section divided into two departments. In the department of mathematics Prof. Forsyth led the way with a brief review of the progress of the calculus of variations during the last century, and in particular of the work of Weierstrass. After referring to later developments of the subject, he gave an outline of the set of four conditions to be satisfied by an integral involving a derivative of the first order of a single dependent variable, and discussed the necessity and sufficiency of these conditions.

Dr. W. H. Young gave an outline of some new results reached by himself in the theory of functions of a real variable. He proved that there could be no difference between the right- and left-hand discontinuities of a function except at a countable number of points, and that a similar result held good for non-uniform convergence. Dr. W. de Sitter, in a paper on a remarkable periodic solution of the restricted problem of three bodies, showed that one of the orbits worked out by Sir G. Darwin is very nearly of the type called by Poincaré a periodic solution *de seconde espèce.*

Mr. H. Bateman followed with a paper on essentially double integrals, and the part which they play in the theory of integral equations. Starting with the integral

equation of the first kind, $f(s)=\int_a^b \kappa(s,t)\ \phi(t)dt$, in which the forms of κ and f are known and ϕ is to be determined, he showed that the solution of this equation was unique if no solution could be found of the so-called "homogeneous" allied equation

$$0=\int_a^b \kappa(s,t)\ \phi(t)dt$$

if

$$\int_a^b\int_a^b \kappa(s,t)\ \omega(s)\ \omega(t)dsdt$$

is essentially positive whatever the form of the (continuous) function $\omega(t)$. Types of such functions $\kappa(s,t)$ are given in the paper, and also a simple proof of a theorem of Hilbert.

Major MacMahon read a paper on operational invariants, in which he obtained several interesting and elegant results in this abstruse department of analysis.

Prof. Love read the first of a group of papers on the best methods of introducing certain fundamental results in analysis. In this he detailed a method of proving the fundamental properties of the exponential function. Starting from the attempt to differentiate a^x. he introduced the number e as the limit of $(1+1/n)^n$ when n is infinite, and by applying the theorem of the mean value to the expression

$$\phi(x)=e^b-e^x-(b-x)e^x-\frac{(b-x)^2e^x}{\underline{|2}}-\frac{(b-x)^{n-1}e^x}{\underline{|(n-1)}}-\frac{(b-x)^n}{b^n}\mathrm{R}$$

where R is the difference between e^b and the first n terms of the series for e^b, he obtained readily the exponential theorem. In the interesting discussion which followed this paper Dr. Young suggested that the concept of an infinite series was really simpler than that of a limit, since the former involved only a countably infinite number of steps, which was not necessarily true of the second. Dr. Hobson emphasised the value of Prof. Love's method as making the student familiar with a type of proof of great generality and power, but Mr. C. S. Jackson deprecated the too early introduction to beginners of difficult mathematical concepts.

In the department of general physics, which proceeded simultaneously with the above, Mr. Sidney Russ read a paper on the transmission of the active deposit from radium emanation to the anode. He showed that, whereas the amount of active deposit obtained on the negative electrode diminishes as the pressure of the air is diminished, as has already been found by Makower, it is found that the amount obtained on a positive electrode simultaneously increases. Experiments have also been made in various gases on the amount of active deposit on the two electrodes, and it is shown that between the pressures of 0·1 mm. and 1 mm. hydrogen behaves differently from air.

Miss I. Homfray detailed a series of experiments on the absorption of argon by charcoal. Miss Homfray finds that a formula of the same type as Bertrand employed for vapour pressures holds for the equilibrium pressures of the absorbed gas at various temperatures and constant concentration. The constants in the equation change with the concentration, and moderately simple equations are obtained expressing the mode of dependence. The resulting equation is much more satisfactory than the experimental formula usually taken.

Sir Oliver Lodge then read a paper on the density of the ether, in which he summarised the arguments for a very high density of the ether which had been given by him in NATURE, March 28, p. 519. His conclusion is that every cubic millimetre of the universal ether of space must possess the equivalent of a thousand tons, and every part of it must be squirming internally with the velocity of light. The latter part of this statement is based on the fact that the existence of transverse waves in the interior of a fluid can be explained only on gyrostatic principles, and the internal circulatory speed of the intrinsic motion of such a fluid must be comparable with the velocity with which such waves are transmitted.

Prof. Trouton showed an electrical experiment illustrating the two modes of condensation of water vapour upon surfaces. If a bell jar be placed over a Bunsen and then placed over a charged gold-leaf electroscope, this often leaks as though the air were ionised. The action is, however, somewhat uncertain. If the insulating shellac of the electroscope is gently dried with a flame, the experiment if made fails, but if the shellac be now moistened and wiped so dry with a cloth as not to conduct, the experiment if made will be successful. The action is therefore due to the moisture, but in the former case it is deposited with difficulty for the same reason that well-dried phosphorus pentoxide absorbs moisture with difficulty.

An important paper was read by Mr. A. O. Rankine on a theoretical method of attempting to detect relative motion between the ether and the earth. If a dumb-bell shaped body shortens in accordance with the Lorentz hypothesis in the direction of the ether drift, its moment of inertia will depend upon its azimuth unless its effective mass changes in a compensating manner. The change in its mass necessary for compensation turns out to be of opposite sign to that which would be indicated by the ratio between longitudinal and transverse mass, as given by any of the current theories. Thus there is either something very wrong in these theories or a real effect arising from motion relative to the ether is theoretically detectable in opposition to the view held by Larmor, Einstein, and others. Unfortunately, it has to be recognised that the effect is too small to be actually detected by experiment. In the discussion Sir Oliver Lodge advised caution amongst the many pitfalls in this difficult subject, and appeared to be in favour of the view that when allowance is made in accordance with a complete theory which holds good for large as well as small velocities, the supposed effect will be found to vanish. Prof. Trouton mentioned another experiment which he was making in collaboration with Mr. Rankine. If a wire changes in dimension with its azimuth, it might be expected that its electrical resistance will simultaneously change. Four coils are connected to form the arms of a Wheatstone's bridge, adjacent arms having their axes at right angles to one another. Balance is obtained, and then the framework on which the coils are mounted is rotated through a right angle. When the temporary disturbance has subsided, the arrangement is again tested for balance. The experiments are in progress, and so far a small positive effect has been obtained, but it requires confirmation before so minute though important an effect can be considered certain.

The afternoon meeting began with a challenging paper by Prof. H. E. Armstrong on the nature of ionisation. This attracted the physical chemists in particular, who appeared with the object of combating Prof. Armstrong's views. These may be summarised as follows:—

We do not "need to imply more by the term *ionisation* than that the medium is in a state in which it will conduct electricity." "The doctrine of electrolytic dissociation is destitute of common sense." Ohm's law is consistent with a modified Grotthus hypothesis. "The assumption that any electromotive force, however small, will condition sensible electrolysis" is a fact which cannot be regarded as established, as it is impossible to avoid some polarisation. Electrolytic conductivity and chemical activity *se confonde* (Arrhenius). Chemical interactions are dependent on "mutual attractive relations of the particles." In effecting hydrolysis in the case of sugars, "enzymes act selectively; therefore their action cannot be attributed to dissociated hydrogen ions." "The mistake has been made that liquids are comparable with gases—a preposterous contention." "'Ionised' molecules are complex, reversible systems formed of solvent and solute under the influence of the force of residual affinity." "As such systems break down under the influence of the current new ones to take their place must arise spontaneously in the solution: the molecules, therefore, would draw one another apart at a rate proportional to the polarisation," and hence Ohm's law would be satisfied. In the discussion Sir O. Lodge emphasised the distinction between the terms "electrolyse" and "ionise." Electrolyse signifies decomposition, ionise means making

ready for decomposition, or, if we like, to decompose and to leave the products of decomposition in the fluid instead of getting rid of them at the electrodes. It really means converting a substance into an electrolytic conductor, and for that process the term is wanted. As to the nature of ionisation, the facts to be expressed are that the atoms are perfectly free to travel, that an extremely minute force will set them in motion. They are either free, therefore, or else potentially free. The merest trace of E.M.F. suffices to practically transfer them in one direction. To account for this, we may suppose a certain proportion of the atoms are either continually or persistently free, which is improbable; or that they are constantly interchanging with others, in such a way that they are free between their combinations; or else that they are combined into such a loose and large aggregate that the attraction in all directions is equal—which is really a case of potential freedom barely distinguishable from actual freedom. If the instinct of chemists really prefers this view to the idea of actual dissociation, then great weight should be attached to that instinct, and the aggregation mode of specification should be preferred; but for practical purposes it makes very little difference, and in treating the matter mathematically, actual dissociation is the idea most easily expressible.

He was not surprised that Prof. Armstrong prefers this hypothesis of aggregation, inasmuch as he has for twenty years preached the doctrine of residual affinity as explanatory of solution, of molecular combination, and of many other such things, as distinct from simple and undivided units of valency. Now this doctrine is entirely consistent with the electrical theory of chemical attraction. For the single bond with which, say, sodium is attached to chlorine is, on the electrical view, not a unit of which no fractions are possible, as perhaps it used to be regarded, but a group or bundle of lines of force which can be split up into any number of fractions, a few lines being easily diverted. Thus a few are thrown off to encounter any water molecules which may come into the molecular neighbourhood; and although the attraction of each water molecule for the sodium or the chlorine must be much less than the attraction of sodium and chlorine for each other, yet in a dilute solution, when the water molecules are numerous, the cooperation of a lot of them may exert a pull sufficient to balance the attraction of the atoms of the salt itself, and either actually or potentially to tear the salt molecule asunder. But whether it is really torn asunder or not, or whether it goes about with an aggregate of water molecules hanging on to it, ready to be torn asunder by the slightest addition of applied electric force, is a matter on which as a physicist, at least from the electrolytic point of view, he would express no opinion. It is possible that some of the phenomena now known to physical chemists may afford a criterion for distinguishing between these two extremely adjacent hypotheses. But he wished to testify to the fact that the residual affinity doctrine, when developed in accordance with the electrical theory of chemical action, is a luminous one, and explains, not only the influence of a solvent, but mass action generally; and that we owe a debt to Prof. Armstrong for keeping the chemical reality of the facts thus able to be explained always before us.

As to the objection to thermodynamical reasoning; he should allow that thermodynamics is a method of arriving at results, but it is not ultimately to be regarded as the most satisfactory method. For it does not pay attention to the process by which the results are obtained: it is rather a method of proceeding blindfold, or in blinkers, and reaching the result by an ingenious scheme of argument without attention to the process involved. It is remarkable that reasoning can be carried on in this way, but it is undeniable that so it is.

So far from chemistry being cold-shouldered by physics, it would appear rather to be the other way; and the great interest of the present state of things is that the facts of chemistry, or at any rate the most fundamental of them, seem now able to be tackled by mathematical methods, and to show signs of being more completely understood than ever before.

This is a process which, however puzzling it may be in its early stages, will surely be welcomed by chemists in the long run; and it will be a great day when such an empirical generalisation as the periodic law really yields up its secret to mathematical analysis—based, as that probably will be, on the hypothesis of the electric constitution of the atom.

With all that had been said about Prof. G. F. FitzGerald he heartily concurred, but he seldom or never found himself in disagreement with that great man; and inasmuch as Prof. Armstrong seems to agree with him too, at any rate to a great extent, there appears to be less difference between the physicist and the chemist than might superficially appear.

Meanwhile, we should not avoid the use of the word "ionisation," but should use it carefully, and should determine the nature of the process which it represents.

Prof. Abegg reminded Prof. Armstrong that combination between solute and solvent is admitted by everyone. But this alone does not explain the duplication of the number of molecules which is indicated by any method of counting, such as lowering of freezing point, osmotic pressure, &c. The difficulty felt by some chemists in realising how simple solution should effect such a thorough change of molecular state is lessened if it is recognised that even in the solid state the molecules are often dissociated. Internal friction is the cause of the small conductivity of solids, not want of dissociation. No theory explains the same number of facts as the dissociation theory. Prof. Werner emphasised the necessity for postulating association as well as dissociation, bringing forward his own work on complex inorganic combinations. Without association there is no ionisation. Dr. T. M. Lowry said he had been led to advocate a hydrate theory of ionisation. The molecule does not fall to pieces, but is torn apart by the affinity of the solvent for the ionic nuclei of which the salt is composed (Trans. Faraday Society, July, 1905). Dr. Burch said chemists and physicists had attacked the subject from opposite ends, like engineers driving a tunnel, and the question was whether they would meet in the middle, and he thought they would.

Dr. Senter directed attention to Werner's demonstration that in such compounds as $Co(H_2O)_6Cl_3$ the water was associated with the positive ion, and that it had also been shown that the positive ions of the alkali and alkaline earth salts were also hydrated to a considerable extent in solution (*cf.* Senter, *Science Progress*, January, 1907). He considered that Armstrong's theory that the variation of electrical conductivity with dilution could be accounted for by hydration alone was opposed to the law of mass action; the requirements of this law will be satisfied only if the molecule splits up into two parts which convey the current.

Dr. N. T. M. Wilsmore claimed that the ionisation theory was a growing one, as was indicated by the recognition to-day of the influence of the solvent. Against a Grotthus theory he urged that, since two molecules are involved, the conductivity would vary as the square of the concentration. Dr. Haber inquired how the theory of a concentration cell could be worked out if no dissociation occurs. He also discussed the bearing of solubility, *e.g.* with regard to the decrement of solubility of AgCl by addition of sodium the quantitative relations are the same as we deduce from conduction experiments. He would express the solution of a salt such as sodium chloride by the equation

$$\mathrm{ClNa} + (+)(-) \rightleftarrows \mathrm{Cl}(-) + \mathrm{Na}(+)$$

where $(+)$ stands for a unit positive and $(-)$ a unit negative charge.

Throughout the discussion the term hydration was employed. We may point out that we believe Prof. Armstrong would not take this term as adequately connoting the formation of the complexes that he postulates.

Two papers were read by Prof. Rutherford, the first on the production and origin of radium, in which he gave evidence that there must be an intermediate substance formed in the derivation of radium from uranium. Sir O. Lodge drew from Prof. Rutherford the statement that he did not know whether or not this substance was radioactive. The second paper, in collaboration with J. E. Petavel, was on the effect of high temperatures on the activity of the products of radium, in which they detailed

experiments made with radium inside an explosion chamber, proving that a *sudden* rise of temperature does not affect the rate of disintegration, but that subsequent effects are due to a change as if of B and C together.

In the discussion on the latter Mr. Makower stated that the experiments afforded a confirmation of his own experiments and those made by him in conjunction with Mr. Russ. The only point in which the experiments do not agree with the view that it is radium C which is affected by temperature is that after the diminution of activity succeeding the explosion the rate of recovery is too slow. This might be accounted for by assuming a sudden change in the activity of radium C accompanied by a change in its period instead of assuming a change of activity in both B and C.

Mr. H. Stansfield read a note on the echelon spectroscope and the resolution of the green mercury line, in which the detection of several faint new components is described; and a paper by Mr. L. F. Richardson, on a freehand potential method, had to be taken as read owing to lack of time.

The proceedings on Tuesday, August 6, opened with a discussion on modern methods of treating observations. This was initiated by a paper by Mr. W. Palin Elderton expository of the methods developed by Prof. Karl Pearson. After defining the principal quantities involved in the new methods, the paper dealt with correlation and its calculation. Examples were given examining possible correlation between rainfall in different districts, rainfall and typhoid in Surrey districts, and other meteorological questions. In the case of rainfall and typhoid, the statistics made use of indicated so little correlation that "it is impossible to assert definitely that there is any relation" between them. In the discussion Dr. W. N. Shaw emphasised the present need of a consideration of "departures from the mean." He directed attention to other methods of treating observations besides that of Prof. Pearson; for example, Prof. Schuster's periodogram method and the "method of residuation" adopted by Prof. Chrystal in discussing the component periods of oscillation of the seiches of Scottish lochs. He showed diagrams, prepared in the Meteorological Office, which illustrated the relation of the mean to the frequency of occurrence of the various values. Mr. G. Udny Yule explained that to be content with the arithmetic mean was to neglect all the other characters in which two frequency distributions might differ. The standard deviation was the most convenient measure of "scatter," but the difference between the two values, which were just exceeded by one-quarter and three-quarters of all the observations respectively, was the most readily calculated. The correlation coefficient was a measure of the approach towards a simple linear relation between two variables, and could be extended to cover more complex cases.

Mr. A. R. Hinks, who was somewhat sceptical as to the general applicability of the new methods, inquired what meaning could be attached to the value 0·3 of the correlation coefficient in such cases as $y = \log x$. He also gave an example in which questionable conclusions had been arrived at by the method, the reason being that certain groups of stars had been studied for special purposes, while others had been neglected. The choice of observations introduced a fictitious law of distribution. The discussion was continued by Mr. Hooker, Prof. Turner, and Prof. Edgeworth.

Prof. Hicks then read a paper on the use of calcite in spectroscopy. In order to be able to make use in spectroscopy of the large dispersion in the ultra-violet produced by calcite, Prof. Hicks first polarises the light in order to do away with the duplication of the spectral lines. When a quartz lens is employed in the collimator it is made a compound lens of right- and left-hand quartz; the polariser for very short wave-lengths (down to λ 2300) was a small Foucault fixed like a comparison prism. The Hon. R. J. Strutt pointed out the great advantages that fused quartz would have.

A series of papers on astronomy and cosmical physics were now taken. Dr. O. Backlund detailed the work done in determining the variation of latitude. Dr. W. N. Shaw read a paper on some recent developments of the method of forecasting by means of synoptic charts. These are the methods of M. D. Gréville, of Paris, and M. Guilbert, of the Meteorological Society of Calvados, for obtaining an approach to a second approximation in forecasting weather. Mr. C. Michie Smith gave an account of the Kodaikanal Observatory in South India. The Rev. A. L. Cortie, S.J., read a paper entitled "The Variability in Light of Mira Ceti and the Temperature of Sun-spots," the purport of which was to indicate the relatively low temperature of sun-spots from the behaviour of the bands of titanium oxide in *o* Ceti, when the star is at two different temperature levels represented by a whole magnitude in luminous power. Concomitant evidence of the variation of temperature of the star was furnished by the character of the hydrogen lines.

Prof. H. H. Turner read a paper descriptive of a method which is being tried at Oxford for improving the constants of the plates for the Astrographic Catalogue. He also read one on the determination of periodicity from a broken series of maxima. For example, observations of the light curve of a variable such as U Geminorum are generally absent altogether near a minimum, and cannot be supplied; moreover, the maxima are often lost from cloudy weather, &c. The paper suggests a method of examining these for the detection of periodicity. Let E_1, E_2, E_3 be the epochs of maxima. Find the differences between these and the *nearest* theoretical maxima E_0, E_0+2n, E_0+4n, &c., where $2n$ is a period to be tried. Find (*a*) the algebraic mean of these differences, (*b*) the sum of their squares. In certain circumstances (*b*) should be a minimum when we have hit on a real periodicity. If (*a*) comes out sensibly different from zero, we must alter E_0 until (*a*) is small enough. When there is no periodicity near $2n$, (*b*) will approximate to $mn^2/3$, m being the number of maxima treated. Miss M. White, T. V. Pring, and J. E. Petavel communicated a note on an analytical study of the meteorological observations made at the Glossop Moor kite station during the session 1906–7, and a preliminary note by W. A. Harwood and J. E. Petavel was read on the recent international balloon ascents (Manchester station). A paper by T. J. J. See, on results of recent researches on the physics of the earth, was taken as read.

Meanwhile, the mathematical department had been meeting separately, continuing the group of papers on the elements of analysis with one by Dr. W. H. Young on the introduction of the mathematical idea of infinity. Dr. Young stated that, of the three types of mathematics, viz. the logical, formal, and practical, his sympathies were with the first and third rather than with the second. He pointed out a number of instances in which learners were brought into contact with the notion of infinity, and advocated the policy of boldly facing the difficulty instead of trying to avoid it. A paper by Mr. C. O. Tuckey on the teaching of the elements of analysis unfortunately had to be cut short owing to lack of time.

Mrs. Boole-Stott exhibited a series of beautifully constructed models of three-dimensional sections of regular hyper-solids in four dimensions, and models illustrating the rotation of a four-dimensional body about a plane. Prof. Schoute showed some lantern slides in connection with this subject. He also exhibited three models of developable surfaces the tangent planes of which are given by the equations

$$u^3 + 3u^2x + 3uy + z = 0$$
$$u^4 + 6u^2x + 4uy + z = 0$$
$$u^6 - 15u^4 + 15u^2x + 6uy + z = 0$$

and indicated certain results when the parameter u in the equation of the plane occurs to a higher degree. Prof. A. M. Worthington showed a series of slides, directing attention to the fact that the impact of a drop excavates a perfectly spherical hollow which reaches its greatest depth at apparently the same time that the water thrown up attains its maximum height. The volume of this pit is enormously greater than that of the drop. The object of the paper was to obtain suggestions from mathematicians explanatory of this phenomenon, but none were forthcoming. Prof. Hilton gave an account of a new property of Abelian groups, and Lieut.-Colonel A. Cunningham read a paper on the factorisation of the terms τ_n, v_n, of the Pellian equation $\tau_n^2 - Dv_n^2 = 1$.

Besides these papers, reports were presented by various committees nominated by the section. Owing to the plethora of papers, these, as a rule, were taken as read, printed copies being distributed to members present.

The committees presenting reports were those denoted by their well-known abbreviated names of "electrical standards," "kites," "Ben Nevis," "Bessel functions," "teaching of elementary mechanics," "Falmouth," and "seismology." An account was given of the last by Prof. Milne.

ANTHROPOLOGY AT THE BRITISH ASSOCIATION.

THE most noticeable feature about the proceedings of the Anthropological Section was the great predominance of papers of an archæological character, those on physical anthropology and general ethnography being far fewer in number than usual. The general level of the papers was, however, quite up to the usual standard, and several of the communications were of the first importance.

As the archæological papers were so much the more numerous, it may be advisable to deal with them first. On the Monday morning an important discussion on the Iron age was initiated by Prof. Ridgeway in a paper on the beginnings of iron. He argued that Central Europe was the true centre of the use of iron as a metal, and that it was first diffused from Noricum. He considered that the general opinion as to the early knowledge and use of iron in Egypt is explained by the fact that hæmatite was known and used, but that it was not treated as a metal, but as a stone. That iron was worked from a remote period in Central Africa he considered unlikely, as it only became known for the first time in Uganda some five hundred years ago, and there was no reason to suppose that it was worked much earlier in the more central part of the continent. As it was also certain that the peoples beyond the Caspian and along the shores of the Indian Ocean did not use iron until a late date, it seemed clear that its use as a metal was due to Central Europe.

In the discussion which followed Prof. Edouard Naville drew a distinction between the knowledge of iron and its general use. Referring to the two or three cases of iron being found of the time of the Old Empire, he pointed out that, in spite of this, it did not seem to be in common use under the New Empire, and that no iron tools were discovered in the Deir el-Behari excavations. His own feeling was that the general use of iron in Egypt was not anterior to Greek times.

Prof. Petrie emphasised the necessity of keeping clearly in view the distinction between the general and sporadic use of iron. Iron was known for 4000 years before its use became general, and this sporadic use strongly supported Prof. Ridgeway's views of the use of iron in its native state, as, had processes of reduction been known, it was unlikely that it would have taken 4000 years for its adoption to have become general.

Prof. J. L. Myres argued that there was no logical connection between Prof. Ridgeway's view that the knowledge of iron, as a useful metal, spread from a centre in Noricum and his assumption that the question of the early Iron age in Europe was that of the first use of iron at all. He pointed out that materials, for example tobacco and gunpowder, were not infrequently looked upon as mere curiosities in one area, and that their real utility was not discovered until they were transferred to another district. He also dwelt on the effect which the introduction of the blast furnace from the north must have had upon the output.

Mr. Arthur Evans considered that the great objection to Prof. Ridgeway's theory was the comparatively late date of the Iron-age civilisation of Hallstatt. Earlier phases are seen in southern Bosnia, and still earlier in the geometrical and sub-Minoan tombs of Greece and Crete. He considered that the general adoption of iron in the countries of the Ægean corresponded with the break-up of the earlier Minoan and Mycenæan type of culture.

Prof. Bosanquet felt that a great difficulty in the way of accepting the views of Prof. Ridgeway was the impossibility of testing the theory that the general use of iron had made its way into Greece from the north, owing to the very little available evidence as to Bronze-age culture in Macedonia and Epirus.

Mr. Crooke considered that India may have been the seat of an independent discovery of the metal.

As usual, Egypt took a prominent place in the proceedings, and the section had the advantage of numbering Prof. Naville among those who read papers. Besides giving a descriptive account of the excavations at Deir el-Bahari, which have now been brought to a satisfactory conclusion, Dr. Naville read an important paper on the beginnings of Egyptian civilisation. The conclusion at which he arrived was that the Egyptians were a nation formed of a mixture of Hamitic conquerors from Arabia settling among an indigenous stock of Hamitic-African origin, an amalgamation made the easier as both races were of the same stock and had no religious differences. Prof. Petrie also gave a paper to the section describing the excavations carried out by the British School of Archæology, under his direction, at Gizeh and Rifeh. In this communication he described the interesting series of pottery soul-houses, found on the latter site, which are of great importance apart from their religious significance as showing the design and evolution of the ordinary Egyptian house, about which little had previously been known.

Greek archæology was dealt with in papers by Prof. Bosanquet and Mr. R. M. Dawkins. Both of these papers dealt with the work now in progress at Sparta, but while Mr. Dawkins gave a general description of the excavations, Mr. Bosanquet dealt especially with the scourging of the Spartan boys before the altar of Artemis Orthia, which was shown by the excavations to have occupied the same position for more than a thousand years. Prof. Bosanquet traced the history of the scourging festival, and showed that the cruel whippings described by Roman writers are an artificial revival of an old discipline which apparently originated in a rough game played by the Spartan youths, in which at first there was no element of passive endurance so characteristic of the later ordeal. This game itself seems possibly to have originated in a still earlier custom, in which the lads hit each other, for luck, with boughs cut from the sacred tree, the *Agnus castus*.

The recent expedition undertaken by the University of Liverpool to northern Syria and Asia Minor was described by Prof. Garstang. The work done was of very great interest, the most important find being what is apparently an altar of dedication, similar to those discovered in Crete. Many inscriptions were also found, as well as a large sculpture of an eagle standing on three lions.

In English archæology Dr. Auden described a series of objects, referable to the Viking age, recently discovered at York. Several of the objects have not previously been reported as occurring in England, and amongst these the brass chape of a sword scabbard, with an interlacing zoomorphic design, is of peculiar interest. The general consensus of opinion is that the finds may be referred to the first half of the tenth century, at which time Scandinavian influence in York was at its height.

The progress of the excavations at Caerwent, including the discovery of the Forum and Basilica, was described by Dr. Ashby, who also, in a paper on Sardinia, directed attention to the *nurhagi* or stone towers and their resemblance to the brochs of Caithness.

Another important archæological paper, dealing, however, with a very different area, was one in which Dr. Seligmann and Mr. Joyce described a series of prehistoric objects from New Guinea. The objects described consisted of stone weapons, engraved shells, and pottery, and are truly prehistoric, inasmuch as the present natives do not know who made them, and in some cases cannot even say for what purpose they were made. It is interesting to note that some of this prehistoric pottery is superior both in make and ornament to that now in use among the natives.

The most important papers on physical anthropology were those by Mr. Gray and Dr. Shrubsall, which opened the discussion on anthropometrics in schools. This discussion was held conjointly with Section L (Educational